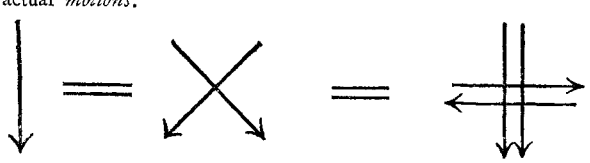
myself the task to prove) there is not really more than *one* explanation of the constitution of the ether in principle conceivable [excluding, of course, the essentially endless vagaries about "forces"], then on this ground alone the hope may be entertained by those who look to the existence of an *explanation* for every physical fact, that difficulties that may naturally present themselves at first will not prove insurmountable by a due amount of thought and careful analysis. S. TOLVER PRESTON

The Transverse Vibrations of Light

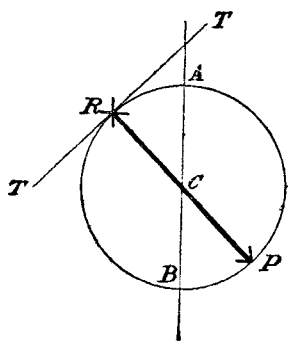
FEW are probably likely to underrate the vast benefits which have accrued to physical science since the time of Poisson, from the application of mathematical analysis to physical problems; but it seems to me we are at present rather in danger of forgetting that such mathematical reasoning can only lead to useful results when founded upon definite physical conceptions. It was upon such a basis that the triumphs of Young, Fresnel, and Airy were won; and it is for want of such a basis that I fear we shall get little aid from Mr. Tolver Preston's ingenious speculation. Mr. Hicks has taken some exceptions to them, which seem sound if he has correctly read the theory, though I am not quite sure he has, or that I should go quite with him in regard to what Mr. Preston *may* mean. But I wish to point out, with your permission, objections of a more simple and definite physical nature.

The only transverse movements capable of being communicated to an ether-molecule by transverse vibrations of matter, which do not involve translation through a measurable though minute distance in space, appear to be rotation on an axis, or (if we conceive the molecule as an annulus) alternate contraction and expansion—"vibration" Mr. Preston calls it—within its own limits. In the case of a gaseous constitution, transverse translation in space with the assumed long free path, must continue, and result in a free path different from the assumed direction of the ray. The particles of sand employed to illustrate the subject thus acquire a continuous transverse *motion* in passing through the sieve, and do not "vibrate" or come back in an orbit. Such true vibratory motion is the main characteristic of solid bodies, and is the best known reason for conceiving the ether as of a "solid" constitution.

What I wish to point out is, that large classes of phenomena appear to demand such actual transverse *motion* in orbits of the entire ether particles, and cannot at present be explained without. I confine myself to two of the simplest examples from polarisation. It is well known that the production of complementary colours from a plane polarised ray by a doubly-refracting film and analyser, may be simply represented to the eye and the mind by the following diagram equation, which shows the resolution of actual *motions*.



Similarly, the two circularly-polarised rays in quartz, and their conversion on emergence into a plane ray rotated on its axis by the angular value of the difference in velocity, may be represented thus:—



Here actual motions in the plane AB are resolved into two opposite circular motions represented by the doubly-barbed circle, which meet on their emergence at the point R, to which their respective velocities from the common departure A have brought them. There they are again resolved, the two tangential forces TR destroying each other, and the two radial forces, RC, uniting in the rotated plane wave, RP. Here again we have throughout actual motions, through definite distances. And I am at a loss to see how whole classes of phenomena of which these are typical can be explained in any other way, or by any but a true "vibration" bodily to and fro in space. If it be so, then it is not enough for some vague physico-mathematical notion to satisfy abstract mathematical conditions; we must ask for the definite physical conception which is to account for the physical phenomena. Until we have this we have made no real advance in comprehending the physics of the ether.

At the same time I cordially agree with Mr. Preston in his regret at the comparative distaste for the study of this subject; and I may, perhaps, add a suggestion on my own part, though not really new. To my own mind it seems as easy to conceive of "matter" without gravity as with it, and of infinite elasticity as of elasticity at all (which is not easy). In ponderable matter, again, the most highly elastic solid bodies are as "solid" as the least; the greater mobility of their atoms by no means interferes with that peculiarity of vibrating *in orbits* and preserving a *locus* which distinguishes solids from fluids, and which so far we have been obliged by the phenomena to attribute to the ether also. Granted that to account for elasticity we have to conceive atoms not in contact, and are confronted by the old mystery of how they can act upon each other across a vacuum. Still, does this confront us any *more* in the ether than in ponderable matter; and, so far as they do go, are not our conceptions of the one sufficient for and equally applicable to the other?

LEWIS WRIGHT

Wellfield, Ashley Road, Crouch Hill, N.

Diffusion of Copper in the Animal Kingdom

JE lis dans la NATURE, vol. xxi. p. 305, un article intitulé "Diffusion of Copper in the Animal Kingdom," se terminant par ces mots: *it is to be hoped that more extended observations will inform us of the exact nature of the rôle played by cupric compounds in the animal economy*. Je crois pouvoir satisfaire en partie au moins à ce vœu. Dans plusieurs communications insérées dans les publications des Académies des Sciences de Paris et de Bruxelles (1878 et 1879), j'ai montré que chez certains mollusques céphalopodes et gastéropodes et chez les crustacés décapodes, le cuivre joue dans le sang le même rôle physiologique que le fer dans notre sang.

Le sang veineux du poulpe (*Octopus vulgaris*), du homard (*Homarus*), etc., contient une substance albuminoïde incolore, cuprifère, à laquelle j'ai donné le nom *d'hémocyanine*, terme rappelant sa parenté avec l'hémoglobine. *L'hémocyanine* forme dans la branchie une combinaison peu stable avec l'oxygène; cette combinaison *l'oxy-hémocyanine* est d'un beau bleu. Elle se décolore en se dissociant sous l'influence du vide ou du contact avec les tissus vivants. Aussi le sang artériel du poulpe est d'un beau bleu tant que l'animal respire une eau bien aérée. Il suffit de comprimer la branchie, de gêner la respiration, pour voir le sang artériel se décolorer.

L'hémocyanine paraît avoir une constitution chimique analogue à celle de l'hémoglobine. Comme cette dernière elle est susceptible de se dédoubler en une substance albuminoïde ne contenant pas de métal et en une substance cuprifère qui paraît former des sels cristallisables analogues aux sels d'hématine.

LÉON FREDERICQ

Liége, le 11 février, rue du parc, 25

Lines of Force due to a Small Magnet

I HAVE been recommended by Sir William Thomson to send you the following construction for the lines of force due to a very small magnet.

The equation to the lines of force due to a very small magnet placed at the origin of co-ordinates and lying along the axis of x is—

$$\frac{y^2}{(x^2+y^2)^{\frac{3}{2}}} = \frac{1}{C} \quad \ldots\ldots\ldots \quad (1)$$

By varying C we obtain a series of similar curves.

Transforming to polar co-ordinates by putting $x = r\cos\theta$, $y = r\sin\theta$, we get for the equation (1)

$$r = C\sin^2\theta.$$

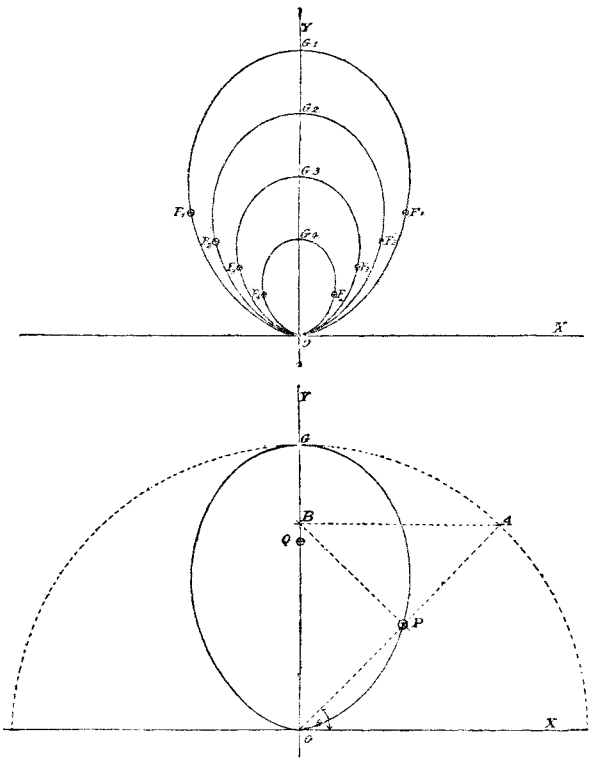
This immediately suggests the following construction:—

Take $OG = C$, and with this as radius, describe a semicircle. Draw any radius OA, then take AB perpendicular to OY, and BP perpendicular to OA. then P is a point on the required curve. Because

$$OB = OA.\cos OAB$$
$$= OA \sin\theta, \qquad \text{where } \theta = \text{angle } AOX.$$

Also $OP = OB \sin\theta$
$= OA \sin^2\theta$.

Therefore P is a point on the curve.



In a similar manner any number of points on the curve may be obtained; and by varying the length OG, we get different curves of the same class.

Near to the point G in the figure, the points on the curve cannot be constructed accurately by the method just given; but if the radius of curvature for the point ($x = 0$) be calculated, it will be found that for the point G it is $\frac{OG}{3}$. Q is the centre of curvature. And a large arc of the circle described about Q with radius QG coincides with the curve. Thus the whole curve may be constructed with great accuracy.

From equation (1) radius of curvature at any point (xy) is given by

$$\rho = \frac{C.y(4x^2 + y^2)^{\frac{3}{2}}}{3(2x^2 + y^2)(x^2 + y^2)}$$

or, with x eliminated by (1),

$$\rho = \frac{C^{\frac{1}{3}}.y^{\frac{1}{3}}.(4C^{\frac{2}{3}} - 3y^{\frac{2}{3}})^{\frac{3}{2}}}{3(2C^{\frac{2}{3}} - y^{\frac{2}{3}})}.$$

Thus $y = .432\,C$ gives the point of maximum radius of curvature.

Lines of force—

$$\frac{y^2}{(x^2 + y^2)^{\frac{3}{2}}} = \frac{1}{C}.$$

These curves may be obtained by giving to C the values

$$C = OG_1,$$
$$C = OG_2,$$
$$C = OG_3,$$
$$C = OG_4.$$

The points (F) are points of minimum curvature.

Glasgow, January 29 JOHN BUCHANAN

Prehistoric Man in Japan

IN an article on this subject (NATURE, vol. xxi. p. 350) by Mr. F. V. Dickins, there is a mistake in dates. He says: "The 'adzuma' or eastern region of the main island was probably peopled chiefly by an Aino race, up to the fourteenth or fifteenth centuries." He hesitates to assign a higher antiquity to the Omori heaps (which were discovered by Prof. Morse) than the thirteenth or fourteenth century, and yet thinks it probable that they were the works of an Aino race. But the fact is that this part of the island was already inhabited by the present race, who had expelled the Ainos long before those periods. Consequently if, as he thinks, the heaps were the remains of the thirteenth or fourteenth century, they cannot be the works of the Ainos; if, on the other hand, they were the works of the Ainos, a much higher antiquity ought to be assigned to them. Such being the case, either one of his conclusions must be incorrect.

London S. SUGIURA

Monkeys in the West Indies

IN NATURE, vol. xxi. p. 131, there is a letter from Mr. Edmund Watt, of Dominica, calling in question the correctness of Prof. Mivart's statement in his paper on "Tails," regarding the non-existence of monkeys in the West Indies.

If by this statement Prof. Mivart means that monkeys are not to be found wild at the present time in any of the West India islands, it is certainly incorrect, as they abound in St. Christopher and Nevis.

If, on the contrary, and what is much more probable, he means that monkeys are not *native* in any of these islands, then he has made no mistake, as I think I shall be able to show.

It certainly does appear remarkable that no species of monkey should exist in the wild state in any of these islands along the whole range from Grenada to Jamaica, with the exception of St. Christopher and Nevis, and the question that naturally presents itself is, Have they been introduced? I am not aware that there is any tradition to this effect in either of these colonies.

It appeared to me that the most likely mode of obtaining information on this point would be to examine all the old West India histories in my possession, as those writers who treated of the natural history of the islands could not fail to notice so singular a fact as the existence of monkeys in two neighbouring islands and in none of the others. The first history examined was that of Rochefort, "Histoire Naturelle et Morale des Antilles, 1665." He names and describes all the mammalia in the West Indies known to him, but no mention whatever is made of monkeys. The next work examined was the "Histoire Générale des Antilles," by Père Du Tertre, 1667, a most interesting book, but little known. Du Tertre was a man of keen observation, and he has devoted a large portion of his work to natural history. He gives a very clear description of all the mammalia with which he was acquainted, but there is not a word about monkeys. This is the more notable from the fact that St. Christopher was considered the mother colony of the other French settlements, and Du Tertre lived there for several years, and visited the island frequently. From the negative evidence afforded by Rochefort and Du Tertre, it may be concluded that monkeys did not then exist in these islands, and, in consequence, must have been subsequently introduced.

On examining a third historical work on the West Indies, that of Père Labat, "Nouveau Voyage aux Antilles, 1744," conclusive evidence was discovered of the *when* and the *how* of the importation of the monkey family into St. Christopher.

Father Labat says that he paid a visit to St. Christopher in the year 1700. He describes the French quarter, the island being inhabited at the time by French and English, and gives a very amusing account of a monkey hunt (*chasse des singes*). He makes the following statement regarding the introduction of monkeys into the island, which I give in the original. "Pendant que les Anglais étoient demeurez maîtres des terres des Francois, dont la plus grande partie resterent en friche, les singes qui s'étoient échapez des maisons des Francois pendant la guerre, multiplierent tellement que quand on reprit possession de l'Isle on les voyoit par grosses troupes. Ils venoient voler jusques dans les maisons, & lorsqu'on plantoit des cannes, des patates ou autres choses, il falloit y faire sentinelle jour and nuit, si on vouloit que ces animaux n'emportassent pas tout ce qu'on avoit mis en terre."

It is thus made clear that the existence of monkeys in St.